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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modeling the feedback between aerosol and meteorological variables in the atmospheric boundary layer during a severe fog-haze event over the North China Plain" *by* Y. Gao et al.

Anonymous Referee #2

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Review of Gao et al., Modeling the feedback between aerosol and meteorological variables in the atmospheric boundary layer during a severe fog-haze event over the North China Plain

The Gao et al. article presents results from the WRF-Chem model run for a particularly severe, regional-scale air pollution episode over the North China Plain in January 2013. The authors compare two simulations from the 'on-line' aerosol/chemistry model: one allowing for feedbacks between the aerosols and chemical fields on the meteorological

fields and a second simulation where these interactions are not allowed. There is a growing body of research investigating the effects of aerosols on the shorter term evolution of the atmosphere and this research adds to that by investigating the influence of aerosol feedbacks on near-surface aerosol concentrations through modifications to the meteorology during this particularly intense pollution episode.

While the methodology and conclusions of the study are logical and well-founded, the one facet of the study that does appear to be weak is the assessment of the model predictions of aerosol amounts and related quantities such as aerosol optical depth against observations. Surface concentrations of PM2.5 are compared against observations for four stations, though all of these stations are in the vicinity of Beijing and are removed from the region with the highest aerosol amounts, judging from the spatial distributions presented in Figure 7. Additional comparisons are shown for aerosol optical depth (AOD) from sunphotometer measurements and point measurements of AOD from MODIS for most of these same stations around Beijing. The only observations further away from the region of Beijing are visibility measurements at Shijiazhuang and Baoding. Certainly nothing can be done about sparse observations, but looking at the observations presented in Che et al. (Atmos. Chem. Phys., 14, 2125-2138, 2014) for this same pollution episode there are sunphotometer measurements of AOD at Huimin, about 300 km south of Beijing and within the BTH region used for spatial averaging of quantities presented in several of the figures. While Wang et al. (Atmos. Envir., 89, 807-815, 2014) present maps of MODIS AOD for this same period, which would help extend the spatial coverage of the comparison of model output with observations. Additionally, Wang et al. use CALIPSO cross-sections of extinction to show that much of the aerosol is within 1 km of the surface for this episode. The vertical extent of the aerosols would be an important quantity to assess for the modeling presented here.

The modeling results and analysis of the 'positive feedbacks' of aerosols on the aerosol concentrations near the surface for this globally significant region are fascinating, however the lack of a thorough comparison with observations makes it difficult to assess

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how accurate the estimate of the magnitude of these effects are. I will note that the comparisons for Xianghe and Tianjin, the two stations furthest to the south of Beijing with PM2.5 observations both show substantial over-estimations of PM2.5 concentration on the order of 30 to 60%. How the overestimations at Xianghe and Tianjin may be related to the model estimations of larger aerosol effects further to the south and west is an open question. A comprehensive (as possible) comparison of model results with different observations would help to strengthen the quantitative estimates of the effects of aerosols on the meteorology during this episode.

Other minor comments are given below.

Page 1096, Line 15: Consider rewording 'a weak weather system'. Perhaps 'weak synoptic-scale winds' or 'a synoptic scale stagnation'?

Page 1098, Lines 10-17: Here it is stated the MADE/SORGAM is used in WRF-Chem and there are several references to the use of a modal aerosol description. On Page 1099, lines 24-28, it is stated that MOSAIC is used and that it is a sectional model with eight size bins. These two statements conflict and it is not clear which aerosol representation was used. Page 1100, Lines 1-4: Are the MOZART boundary conditions specific to the period being studied or are they climatological? It should be made clear in the article. Note a spelling error on 'MOZART'.

Page 1103, Line 24: In place of 'nowadays' I would suggest 'present-day'.

Page 1106, Line 15: Figure 6a presents the difference in the surface energy budget between the two simulations averaged over the BTH region. There is a lot of focus on the Jan 10-15 episode of extreme PM2.5 concentrations, but it is not at all apparent that Jan 10-15 was much different than the rest of the month. Do you have any ideas why the average over the BTH region does not show a significantly larger signal for Jan 10-15? From the magnitude of the differences plotted in Figure 6a, I assume these numbers are averaged over the full 24 hours each day. Does it make any difference if only the 09-18 LT times are used?

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Page 1106, Line 17: The reference to 'net radiation (LH+LW+SW+SW)' is not quite accurate because not all of the terms included there are radiation. Perhaps 'net energy flux'?

Page 1110, Lines 8-10: What does the statement 'The higher the surface PM2.5 concentration is, the greater the increase is in the surface PM2.5 concentration...' refer to? Is it the average differences between Beijing, Tianjin and Hebei, or the temporal behaviour of the differences for each station? If it is the temporal behaviour, the fact that at the time of some of the highest peak concentrations in the EXP_NOEF simulation there are lower PM2.5 concentrations in the EXP_CTL should be acknowledged. I believe this is just a result of the internal variability of the meteorology in the simulations and does not impact any of the conclusions of the paper. However the statement should be clarified.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 1093, 2015.